

# Reinventing the Stanford Medical School

## A Conversation with Henry S. Kaplan

*Henry Seymour Kaplan, who served as chairman of Stanford Medical School's Department of Radiology from 1948 to 1972, died at his Stanford home on February 4, 1984—only four months after he had been diagnosed with lung cancer. Ironically, he was the victim of the same disease that had killed his father when Henry was 15 and against which he had fought so effectively throughout his professional life.*

*When I heard the news, though without knowing the grim prognosis, I contacted him for an interview. I asked if there was any hope that he might be able to beat this form of cancer. He told me that there was no chance of getting out of it alive, then added philosophically that all of us will eventually wear a toe tag, but the difference between not knowing when you're going to die and having had a group of specialists say that you are going to die is huge.*

*I interviewed him on December 7, 9, and 14, 1984. We talked about his role in reinventing the Stanford Medical School. He said three visionaries were key to this transition: pediatrician Robert H. Alway, dean of the medical school; Avram Goldstein, chairman of pharmacology; and Stanford President J. E. Wallace Sterling. We also talked about radiology and radiation therapy and their development at Stanford. Excerpts from these interviews follow.*

—Spyros Andreopoulos, Director Emeritus,  
Office of Communication and Public Affairs  
Stanford University School of Medicine



LANE MEDICAL ARCHIVES

Henry S. Kaplan, who died in 1984, was chairman of the Department of Radiology at Stanford's School of Medicine from 1948 to 1972 and was intimately involved in the school's move to the Stanford campus in 1959.

**Q:** *Let's begin with the history and see where it takes us. Then I'll pose some specific questions.*

**A:** I can't recall anymore all the dates with precision, except that I arrived at Stanford on September 1, 1948. At the time, the Stanford Medical School was physically divided. All the clinical departments were at Stanford Hospital in the city [San Francisco], but there was a small unit on campus that included anatomy, physiology, and pharmacology, as well as a so-called division of biochemistry in the Department of Chemistry, manned by two people, Hubert Loring and Murray Luck. The Radiology Department was in San Francisco, in a little corner of the old Lucy Stern Building.

**Q:** *What were the conditions in which you worked in those early years?*

**A:** They were godawful. Our physical plant was indescribably bad. There had been a famous engineer at General Electric named Snook. He had designed X-ray machines in the 1920s, and Stanford had bought

some of them. On the day I arrived at the Stanford Radiology Department, I observed that his machines had an extraordinary feature—bare wires, carrying up to 200,000 volts, dangling above the patient. We had three nonshockproof machines for radiography and another for fluoroscopy. I insisted that money be provided to change all that, and the dean was as good as his word. But it took several months to do it, and I had more nightmares than in all the rest of my life put together. Every single one of those nightmares, no matter what form they took, always ended in a brilliant blue-white flash. Until we moved these machines out of the department, I just couldn't rest.

Wally Sterling became president of Stanford in 1949. By 1952, if my memory is correct, he had heard arguments on both sides of the issue about moving the medical school from San Francisco to the main campus. He decided to form a committee, but he didn't pick its membership from the old guard, which at the time included people like Arthur Bloomfield in medicine, Harold Faber in pediatrics, and Emile Holman in surgery. Bloomfield, Faber, and Holman had already served on a committee which, a year earlier, had rejected the idea of moving the school, based on the notion that there weren't going to be enough patients. But that was a very static view of things. The Faber committee didn't project forward into the next decade or two. In addition, the old guard in the medical school really were a bunch of pure clinicians—all very good in their fields, but with essentially zero track record in anything that could

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Kaplan, shown here in his office in 1971, served on a committee, formed by Stanford President Wallace Sterling in the early 1950s, that built a strong case in favor of the move.

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remotely be considered research. To them, therefore, it meant nothing that the medical school would be located on the main campus, working side by side with chemistry, biology, physiology, physics, etc.

For his new committee, Sterling reached out for some of the younger faculty, of whom I was one. Indeed, I was the youngest, because when I accepted the job at Stanford I wasn't quite 30 years old. Lloyd Dinkelspiel, who was then chairman of the Board of Trustees, was also a member of the committee, which often met in the evening at very attractive places like the Bohemian Club—places I had no knowledge of, and I was quite overwrought, especially when I looked at the prices on the menu.

We reviewed demographic charts with projections for Santa Clara and San Mateo Counties, and it was perfectly clear that, although we might initially have some paucity of patients, ultimately we would have more patients than we could ever have in the city.

Our committee—which Sterling really ran, because he knew where he was going—built a very strong case for the move, and one of my assignments was to build the general argument about the growing interdependence of the sciences, the increasing scientific basis of medicine, and the fact that the medical school could not flourish unless it was located in proximity to the scientific departments of the main university.

So that decision was firmly taken. And of course, almost immediately there were howls of outrage from people like Holman, Bloomfield, and Faber. To a degree, they were all gentlemen, and their howls were rather restrained, but they were forceful. They kept after Wally, through the alumni, saying this was the worst decision imaginable—it would mean the death of the medical school, and so on. And I'd say Wally deserves a medal for the courage he showed in sticking with that decision.

I might say, parenthetically, that all of this happened in a way that was most painful for me, because we had a stunning house high up on a hill in Sausalito, about a thousand feet above the water, designed by Mario Corbett. It was like a piece of sculpture. I had labored long to develop the landscape, and I absolutely loved the place. But I saw no alternative to the change—the school must move down to campus if it were to try to become anything but a third-rate clinical medical school. It was sort of second-rate clinically then, and it would have slipped backward in time.

**Q:** *Was there any indication at the time that the proposed move of the medical school might not go through?*

**A:** Yank Chandler, who was a fine dean, could not live with the decision, and he resigned. I thought this was an honorable way to behave. However, his successor, Windsor Cutting, a professor of pharmacology, was a very bad choice. Cutting wanted to build mediocrity, in the absence of argument and debate. While Cutting was down on the campus wheeling and dealing, committing the faculty in ways that would pose great difficulty, I got a call from Wally Sterling asking if he could see me privately. Much to my amazement, he asked me what I thought of Cutting as dean. I said he was an embarrassment and a catastrophe. And Wally said it had taken him much too long to come around to the same point of view. But he now felt that way and he was going to ask Windsor Cutting to step down. He asked me who, among the existing faculty, would make a good dean. I paused briefly and I said that, in terms of having integrity and the respect of the faculty and some dynamism, Robert Alway would be my choice, but I thought it would be very tough on him, because he didn't make decisions easily. So Wally appointed Alway acting dean for six months and then made him permanent dean.

That was in 1957. Meanwhile, I served as chairman of the committee to find an architect [for the new medical school]. It was a difficult business. In the end, we had several groups of architects to choose from. There were local architects who had built buildings of distinction but had never done a hospital or a medical school. There were the hospital architects as such, and we ruled them out because we knew their imaginations were very limited. They worked with old formulas. And then there was a group we called the great solo architects, which included



Robert H. Alway, right—a pediatrician who served as acting dean of the Stanford Medical School from 1957 to 1958 and dean from 1958 to 1964—was, according to Kaplan, one of the “visionaries” responsible for the transition.

Eero Saarinen, Edward Stone, and a man in Boston whose name I seem to have obliterated.

When I went to visit Saarinen in Bloomfield Hills [Michigan], where he lived, he took me on a personally escorted tour of the new General Motors Technical Center, which at that time was considered one of the wonders of the architectural world, though it had not been designed by a big outfit. I was deeply impressed with Saarinen. He was a wonderful man, very kindly and soft-spoken. When we got back to his office, he said, “I’m very interested in your project. I have never designed a hospital. There are of course guidebooks and brochures from the Public Health Service—every architect has access to those. It would be a great challenge to design a truly beautiful hospital and medical school.”

But, he added, there was a problem. His firm was busy designing a complete campus for some small university and renovating the harbor of New Orleans, and he went on describing one project after another. The consequence of this was that he could not leave his office. “If you decide that you want me to work with you,” he said, “then delegations of your faculty would have to fly



Arthur Kornberg came to the Stanford Medical School as chairman of the new Department of Biochemistry in 1959, the same year he won the Nobel Prize in Medicine for elucidating how DNA is built.

to Bloomfield Hills every two or three weeks to review progress and help refine our thinking.”

My heart sank on hearing this news. I knew his proposal would be quite unacceptable to the faculty and it wouldn't work. But I must tell you, Eero Saarinen was a fantastically impressive figure. Unfortunately, he died of a heart attack sometime thereafter [in 1961].

After this, I went on to see the architect in Boston. I wasn't impressed with him, and that's maybe why his name doesn't register with me anymore. In successive days I flew to New York and visited Skidmore, Owings & Merrill. Some smooth-talking vice president was immediately available to take me to lunch and tell me all about their vast design and engineering resources and so on. But I knew there'd be some nameless, faceless person in the back room doing our work, while we would be dined and wined by the brass out front. That was not what we wanted, because we would have gotten a carbon copy, in slightly different size, of some of the other hospitals they had done, and they had done plenty.

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Next I went to see Edward Durrell Stone. When I arrived, the chancellor of Vanderbilt University was there, finishing up a visit. Stone had been doing some new large hall for Vanderbilt. I looked at the design, and it was just beautiful. When the chancellor left, Stone and I sat down. I told him about the scope of our project. I asked him whether he had ever done a hospital. He had done two, one in Lima, Peru—a huge obstetrical hospital, which is still there and looks quite good—and the University of Arkansas Hospital and Medical School, which, he said, we were welcome to visit. He had developed a very novel arrangement of beds in relation to nursing units, which worked out quite well and lent itself to a beautiful-looking building. While we talked, I told him we were interested in building about a 400-bed hospital, a complete set of clinics, and classrooms and research laboratories for students and faculty. He said, “If I were offered this job, it would be the biggest single design job that I have ever undertaken in my life. I will put my New York office on a skeleton force, and I will move my family out with me and guarantee to stay in the Palo Alto area for no less than 18 months until the project is well off the ground. After that I will make arrangements for continuing supervision by one of my staff.”

The difference was so night and day. When I returned, I reported all this to the committee, and they voted unanimously to select Edward Stone. Then Wally and the trustees said the Palo Alto City Council members needed convincing, and a junket was arranged to Arkansas. I went along and saw the building. We were all most impressed, and it was clear that the councilmen and the university were ready to give the job to Ed Stone.

It was at this point that Stone approached me and said there was something he had to tell me. He said he was a member of Alcoholics Anonymous, but that he had not had a drink in several years. He said he didn't think he'd fall off the wagon on this job, but he would not accept the offer unless Wally Sterling knew this and was willing to make the offer in spite of it. Here was a man, staring at more than \$22 million worth of buildings and willing to lay it all on the line because of this question, which I thought showed great courage and integrity.

**Q:** *What difficulties did Stone face in this project? I saw some blueprints that looked different from what we now have. Who changed his plans?*

**A:** I'm glad you asked that. It brings up a very important and absolutely true episode. Stone's original design for the medical center included a vertical, high-rise hospital, intersecting with a clinic building of four stories, with the medical school at the other end. It would have taken a totally different shape, saved miles of walking, and most of all, it would have cut down about 10 percent in personnel costs. He took the plan to the Board of Trustees at the time David Packard was head of the board. And so help me God, Stone was voted down. That was it.

We went to the Stones' house that evening for dinner, and he was in despair. He had fought hard with the board on that issue, and he had lost. He said, "Henry, you watch and see. In the course of time there will be thousands of people who will say this goddamned thing is so spread out, and I have to walk my legs off in it, and everyone of them will blame Ed Stone and not the Board of Trustees." It is also not widely known that when the hospital building committee wanted to save costs by deleting the beautiful hanging gardens, Ed Stone shamed them out of it by offering to pay for them out of his own pocket.

I don't know if this is a well-known fact, but the trustees of Stanford University had originally budgeted the total sum of \$6 million for the entire medical center project—a project that included a library, a medical school building, a hospital, and clinics. The final figure, of course, turned out to be \$22 million, and that was accomplished by leaving out one of the important buildings for the clinical sciences, which was eventually constructed four years later. In a sense, therefore, this medical center and school were built by mistake or a ludicrous misunderstanding of anticipated costs.

**Q:** *What role did you play in the recruitment of faculty before the medical school moved to the campus?*

**A:** My role had to do mainly with two faculty members—Arthur Kornberg and Joshua Lederberg. Arthur brought his entire team—what was then the Department of Medical Microbiology at Washington University in St. Louis—with him when he came to Stanford in 1959. I suppose I can take some of the credit for that. It happened this way. I had been invited to Harvard to become the

Cook Professor and Chairman of Radiology of all the Harvard-affiliated hospitals. It was a tempting offer, and it came at a time when I was losing my conviction that the new school, when it moved down, really would become a first-class scientific school. I felt that way because I saw no action on the part of anybody to recruit a chairman for the new Department of Biochemistry, which did not exist except as a small division in the Department of Chemistry, with a very limited research base.

Fred Terman, who was then university provost, eventually became chairman of a search committee to recruit the biochemistry chairman, but by then I was beginning to lean more toward accepting the position at Harvard. Leonard Schiff, a very dear personal friend who was then on sabbatical leave in Paris, sent a very anguished telegram to Wally Sterling, saying, "For Christ's sake, do something, don't let this happen." In time I was asked to come down to the campus and see

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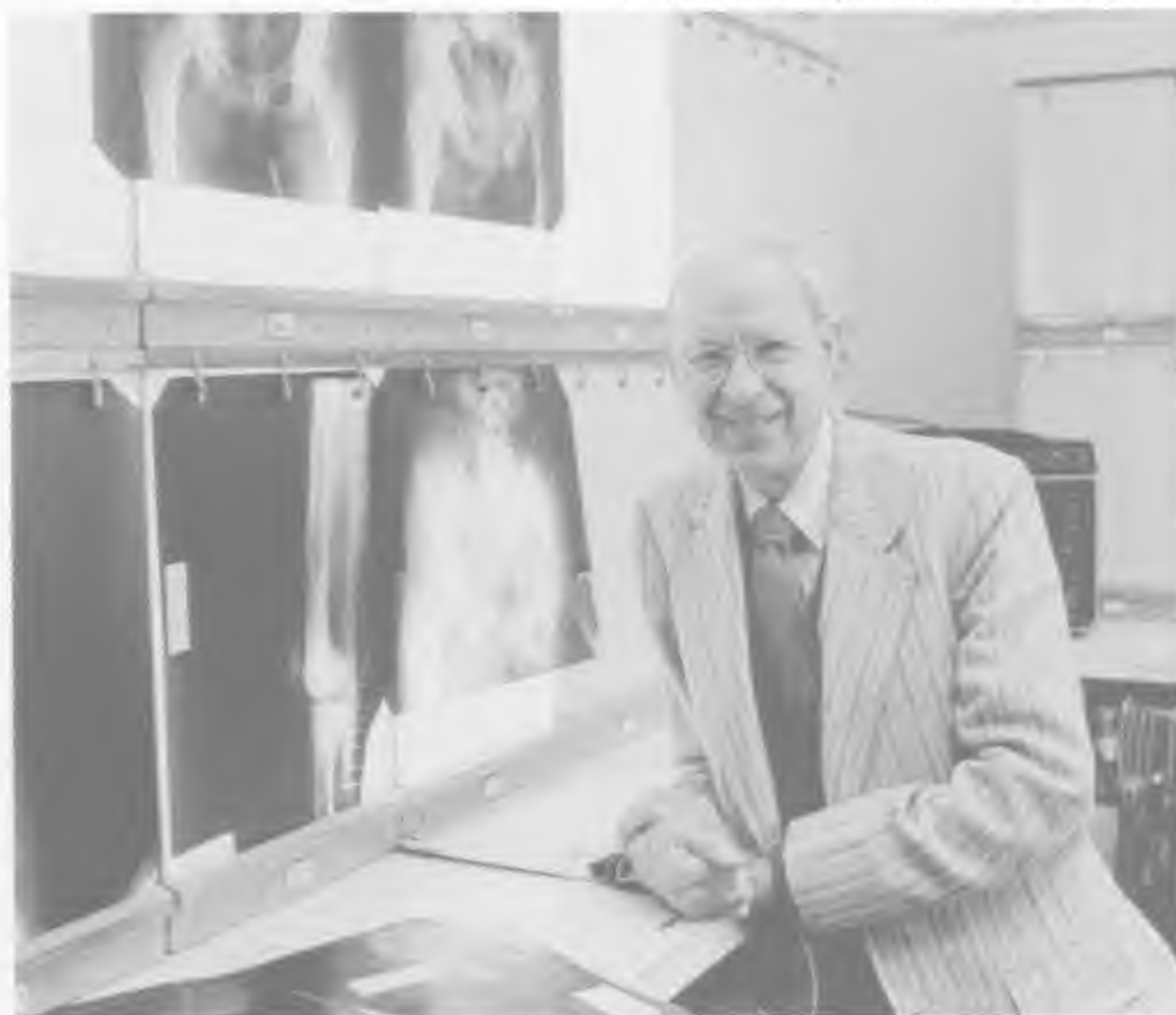
Joshua Lederberg, who had won the Nobel Prize in Medicine in 1958, also came to Stanford Medical School in 1959 as chair of the Department of Genetics.

Terman. As we talked, I was amazed by his negative attitude, because it was clear he didn't think he could compete with Harvard. But I made it clear that I really didn't want anything for myself. I was already well on the way to building a fine department, and anything I needed I could get in the normal course of events. I told him that what I wanted was intellectual "playmates"—that was literally the word I used. I said the medical school had been so strongly clinically oriented—and I have no quarrel with strong clinical care, because I believe in it and practice it—but I also believed in basic research in order to make future care better than what we have today. "For example," I said, "there's biochemistry. We've talked about it now for two years, but there has been no action that I know of."

"On the contrary," Terman said, "I am the chairman of the search committee." He reached in the drawer and fished out a list, and there was Arthur Kornberg's name at the top of it.

I said, "That's wonderful, because Arthur is a dear friend and one of the greatest biochemists in the world." But I added that having him at the top of the list and getting him to come to Stanford were two different things. I said, "I hope you'll pardon my being cynical, but I've watched how Stanford does some of its recruiting.

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In 1948, Kaplan and Henry H. Jones, shown here in 1985, were the only faculty members in Stanford's Department of Radiology.

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Typically, from what I've been able to observe in some other recent episodes, you wait until you hear that the guy has been invited out to give a seminar at Berkeley, so that you won't have to pay his plane fare, and then you'll invite him across the bay to the Farm, and you'll talk to him about the sunshine, the climate, and the bay, and you won't offer him any budget or any space, and then you don't understand why he won't come."

Terman turned kind of green, because this was a rough way to talk to a provost, especially one as tight-laced as Fred Terman. But I didn't give a damn—I had nothing to lose. He said, "What would you like us to do?"

I said, "I'd like you to invite Mrs. Kornberg with Arthur on the first visit. I'd like them to come first-class. I'd like to have a car waiting for them at the airport, preferably a convertible. I'd like them to have a suite at Rickey's. All these are just creature comforts, but they make a difference, because they indicate the level of your interest. Beyond that, I want you to promise him every square inch of space and every dollar of budget that he asks for, because I know Arthur well enough to know that he won't ask for more than he can use."

As I got up to leave, I said to Terman, "I'll make it very simple for you. If you recruit Kornberg successfully, I'll stay. If you fail, I'm going to Harvard." It was just as simple as that. Terman's jaw, of course, by this time was hanging kind of slack. But he was as good as his word. A few days later, Arthur and Sylvie came out, and before he returned home, he had accepted the Stanford job.

A few months after that, I was returning to Stanford from Bethesda by way of St. Louis to offer whatever help I could to Arthur in getting his department ready for the move to Stanford. There was a party that evening at Paul Berg's to celebrate this great new venture. Just as we walked in the door, the phone rang. It was Lederberg wanting to speak to Arthur.

Lederberg simply wanted to know what was going on at Stanford. “I was out there last year looking at a job in biological sciences,” he said. “It seemed like the same old sleepy place as ever, and I turned them down. But now I hear that Henry is staying and you are going. I want to find out what’s happening to inject this degree of excitement.”

We took turns on the phone and talked about the new and improved curriculum, outstanding students, and so on, and Josh said, “Gee, that sounds wonderful! I’d be very interested.”

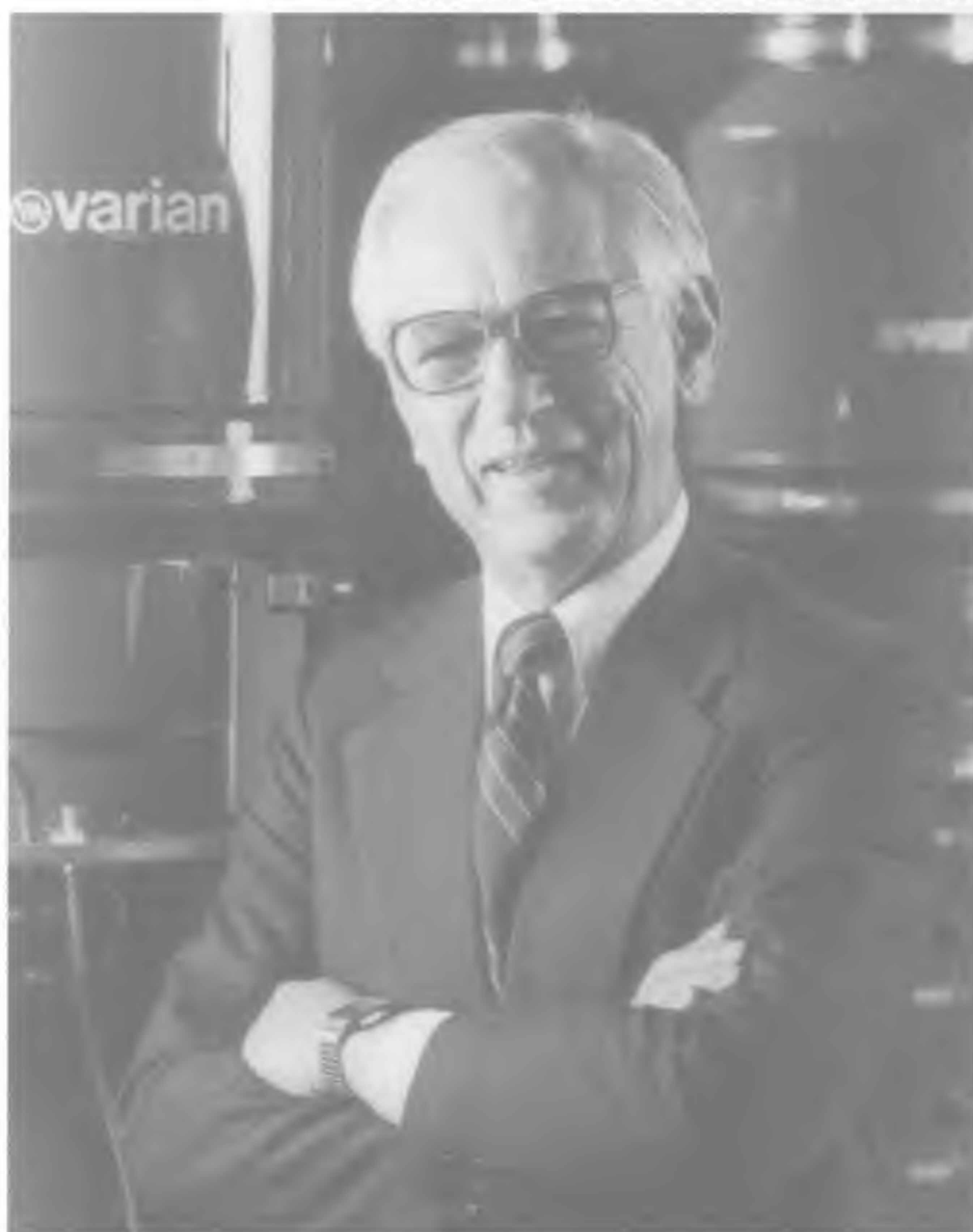
I flew back to Stanford that night, and the next morning I went in to see Bob Alway. This time I didn’t request—I demanded—that he forthwith create a new Department of Genetics, which didn’t exist in the medical school, with Josh as chairman. Alway had to dig down into some temporary dean’s funds in order to get it off the ground. To his everlasting credit, he was never afraid of people who were smarter than he was. And he was not afraid to do battle with the university administration if the issue seemed important enough.

That’s how it happened. And, of course, Lederberg’s and Kornberg’s two Nobel prizes came a year apart, at just about that time [in 1958 and 1959]. Because of that, suddenly Stanford was catapulted, literally, from a second- or third-rate clinical school into some kind of mysterious but very exciting place that students thought of in the same context as Harvard.

**Q:** *During your years as chairman you built a department of radiology noted internationally. What was the secret behind this development?*

**A:** If I had to focus on some central theme in my endeavors, you might say that I created the idea of doing laboratory research in an academic department of radiology. When I arrived at Stanford Medical School on September 1, 1948, there was not a single department of radiology in the world that was doing any scientific research. The only thing being called research was having diagnostic radiologists sitting in front of a viewing box, looking at film, and collecting perhaps one or two cases of some rare malformation.

In those days Henry Jones, professor of radiology, and I were the entire faculty. He was loyal enough to be willing to stick around on an instructor’s salary and



In the early 1950s, Kaplan and Edward Ginzton—a Stanford physicist and co-founder of Varian Associates—formed a committee to look at potential medical uses of the linear accelerator.

work like a dog on Saturdays and Sundays to help get the job done. I did the same. I was finally able to recruit somebody a year later and begin the process of bringing more people in and weeding out those who lacked research orientation.

As department chairman, I felt it was very important for therapeutic radiology to have an experimental as well as a clinical research base. So I insisted on laboratory space from the start. And at one time I made a desperate attempt to persuade Dean Yank Chandler to give me additional space. I pointed out that in the small space we had, there were 28,000 mice, three technicians, and two physicians, which added up to 28,005 breathing, living organisms in about 700 square feet. I made it clear that this was not feasible. He laughed so hard that he gave me the space I asked for.

In a sense, I had a laboratory from the start. I felt intuitively that it was no good having a department



The first patient Kaplan treated with the medical linear accelerator was a two-year-old child with retinoblastoma of his eye. The treatment was successful.

chairman make big speeches about how important research is if he himself did no research whatever. I felt, on the contrary, that in radiology—which was then so backward with respect to research—the only way it would work was for me to see just as many films and patients as anybody else, and to be chairman on top of that and do research. That would shame my faculty into feeling that they, too, could and should do research. Obviously, not everyone I selected turned out to be a red-hot research man. But in the course of time I weeded out most of the ones who were not productive in both areas—the laboratory and the bedside. No matter what people were doing in their research, no matter what else was going on, it was a given that the quality of patient care in the department had to be absolutely first-class. We would not undercut the quality of patient care just to find more time for our research.

Meanwhile, in the early 1950s, another development became extremely important. I began to hear cocktail party conversations about an interesting new atom smasher being developed on the campus by Bill Hansen,

Edward Ginzton, and their colleagues. I became increasingly interested because of the properties of this machine [which could focus intense X rays at deep-seated cancer tissues]. In the fall of 1951 I asked Yank Chandler to introduce me at a luncheon to Fred Terman, who was then dean of the School of Engineering; Leonard Schiff, who was chairman of physics; and Edward Ginzton, who had replaced Hansen after he had died of a lung disease [caused by inhaling the beryllium used in his research].

Ed and I spent our time at that luncheon talking about the properties of the linear accelerator and what we could do. One of the nicest things about it was that you could get very high energies out of it for a very low energy input. You accelerated electrons with these high energies, and you could hit a heavy metal target and make them into X rays. I explained to Ed that radiotherapists had been dreaming about that for years. We needed much higher energies than we could possibly get from the crude devices then available.

Here was a microwave device which—by the clever positioning of the electron pulse, just ahead of each microwave peak—could force that pulse to ride along the microwave at almost the speed of light. The farther it went, the greater energy it got. This sounded miraculous, and I became convinced that this would become the radiotherapy machine of the future. Not only had I convinced myself, but by the end of that luncheon I had convinced all of them. And so Ed Ginzton and I formed a committee to look into the possible applications of the linear accelerator in radiotherapy. But then we ran into problems.

There was an MIT professor, whose name is no longer important, who was a stockholder in the company that made Van de Graaff machines. Every time we submitted a grant application to the government, he knocked it down. It took us more than two years in the richest country on earth to raise \$150,000. Hard to believe. Of course, in those days I had no wealthy contacts. I was strictly dealing with the American Cancer Society and the National Institutes of Health. But we got started with some additional help from the Office of Naval Research. Ginzton was in charge of the design and construction of the machine. I used to come down to the campus from San Francisco as often as I could, to meet with him and members of his staff to see where things were. I participated, of course, most actively in the clinical



design, because I was interested in having a machine that was functional therapeutically, not just a gadget.

By 1955 we had finished our first linear accelerator for radiation treatment. We were about six months behind the British, who had built a machine totally different from ours. The delay was, of course, because of the time it took us to raise the money. We began steps to have our machine assembled in the department in San Francisco. But there was a new problem. Before we could assemble the six-million volt machine, it was clear we had to have protection from its beam.

Wally Sterling came to our aid. He went to the Irvine Foundation and they met with us. He made an impassioned speech, and I made an ordinary technical speech. At that time it was publicly known that the medical school was preparing to move to the campus within four years. But I said to the Irvine Foundation people that if they helped us, we could learn a great deal in four years of work in San Francisco, because the techniques of treatment with this machine were different. "This is like a rifle," I said, "whereas the past machines had been like shotguns. And we're going to have to learn our profession all over again. We're going to have to work out technical procedures that have never been done before." The opportunity to have four years to do that before moving the machine was crucial to us.

The Irvine Foundation made a one-time gift of \$75,000. We built a tiny concrete pillbox into the hillside. It was well protected and had a hatch on top, because we knew that someday we would have to bring a crane along to pick that machine right up through the ceiling and move it to Palo Alto.

The great day finally came and we were ready to turn on the machine for a patient. The true story of what happened is somewhat stranger than anything I could have made up. It was quite unbelievable. We had had many bull sessions of what kind of cancer patient would be our first referral. The very first patient was a [two-year-old] baby with retinoblastoma of his only remaining eye, because surgeons had already removed the tumor in his other eye. This was one of those genetically determined tumors, since his father had had it. I don't think I will ever forget the puzzled look on the face of the garage owner down on Fillmore Street when I asked to borrow a heavy-duty automobile jack, then explained that it was

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to carry a huge block of lead with a pinhole in it, and that we would position that pinhole, day-after-day for six weeks, directly opposite the tumor in the baby's eye, while missing the lens and the cornea. That boy is now in his twenties and doing very well, and his vision in the treated eye is intact.

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*Henry S. Kaplan was chairman of Stanford's Department of Radiology from 1948 to 1972. During his career, he also served as president of the American Association of Cancer Research, the Association of University Radiologists, the International Association for Radiation Research, and the Radiation Research Society.*

*Spyros Andreopoulos is director emeritus of the Stanford University Medical Center's Office of Communication and Public Affairs.*